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HYDROPHOBIA

AND

M. PASTEUR:

BEING AN

EXPOSITION OF M. PASTEUR'S PROPHY-
LACTIC METHOD OF TREATMENT.



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HYDROPHOBIA AND M. PASTEUR.

I.

M. PASTEUR'S EXPERIMENTS ON THE LOWER ANIMALS.

I HOPE it will not be considered presumptuous on my part when I say that M. Pasteur has by no means proved that the measures he advocates as a prophylactic against hydrophobia are successful. Indeed, he has not yet advanced one single fact that entitles him to claim proof on scientific grounds. Some experience of animal poisons has convinced me of the danger of accepting statistical data other than those based upon precise and repeated experimental research as evidence in favour of any theory, preconceived or otherwise. Fallacies are so apt to be lost sight of in the mist of prejudice, and are the rocks on which many a plausible theory has been wrecked. The warning sounds of the bell-buoy may reach the ears of the man at the helm, but success in life may have made him too confident of his ability to steer clear of the rocks, until at last the mist grows thicker, and, in attempting to steer clear of one to which he had gone perilously near, he runs clean on to another. And then people admit that after all the wisest and most experienced of us is not infallible ; but they mourn little for the lives that have been lost. I will endeavour to indicate from my point of view a few of the more salient points which appear to require careful consideration by all those who are desirous of arriving at a just conclusion as to the merits of M. Pasteur's method. The first question which suggests itself is, on what ground does M. Pasteur base the value of his procedure ?

Now, this question, so simple in itself, is not so easy to answer owing to the manner in which M. Pasteur has, from time to time, shifted ground. So much has this been the case that Dr. Dulles, a well-known American authority on the subject of hydrophobia, makes the following remarks :—" If you take the trouble to place side by side Pasteur's statements at different times, we see that they are so inconsistent that the cordial acceptance of almost any one of them seems to demand that the preceding ones should be banished from our memory." Taking a broad view of the matter without at present entering into a discussion as to the inconsistencies to be found in his assertions, we gather that he bases the efficacy of his method (*a*) upon his experiments on the lower animals, and (*b*) upon his subsequent experience in the case of the human subject. In 1874, Pasteur announced that, by weakening his virus by transmission through monkeys, and by strengthening it again by transmission through rabbits, he had obtained a virus which was protective to dogs, and which would eradicate rabies from the world. But, judging from his own experiments, he had never succeeded in rendering "refractory," as he calls it, more than fifteen or sixteen out of twenty dogs. He subsequently abandoned the monkey which he had chosen on the grounds of physical resemblance to man, ignoring the fact that there may be a vast difference physiologically.

Does the result, then—that fifteen or sixteen out of the twenty dogs inoculated remained unaffected—in any way warrant the assumption that the method adopted by Pasteur was protective? The answer is, certainly not; and on this point I will quote Mr. Alfred Poland (*Holmes's Surgery*): "As regards the activity of the poison" (rabie), "it is more volatile and less active than many other morbid poisons, hence all that are bitten do not suffer. Thus

Hunter gives an instance in which, out of twenty persons bitten by the same dog, only one received the disease. Faber states that, at Wurtemberg, of one hundred and forty-five persons bitten, twenty-eight only were attacked. It is generally calculated that, of those who are exposed to the venom, about one in four matures the complaint, and the rest escape." Far from being evidence in favour of his method, the results of his experiments tend to show how powerless it was to influence the operation of the virus. A precisely similar fallacy led Professor Halford astray, some years ago, in reference to the intravenous injection of ammonia for snake-poisoning; and, indeed, there appears to be much in common in the reasoning by which infallibility is claimed for the respective modes. The edifice erected by Halford was demolished by the first blow from the arm of exact experimental research, and I venture to predict that the same fate awaits the gigantic, but unsubstantial, edifice which is still being erected by Pasteur. Let us hope that his reputation will not be buried in the ruins. A writer in the *New York Herald* says: "And now, when we look back over the whole of M. Pasteur's work in connexion with the subject of hydrophobia, how shall we judge it? The final impression is—to say the least—disappointing. In spite of the distinctness of his assertion and belief that he has solved the problem of the cure of hydrophobia, or of its prevention at any time before its outbreak, when we try to follow the steps by which he has reached this conclusion, we find ourselves in bewilderment. We seem to be in a maze, travelling blind-folded, cheered here and there by his assurance that all will be well. At length he seems to say 'Look about you, we are safe.' But when we look about, the safety does not appear. The proofs of it are still nothing but his bare asser-

tions. When we examine these, we find they are in part contradictory of each other, while in part they contradict facts of which he does not seem to be aware. When we seek for evidence that he is familiar with the clinical manifestations of what is called hydrophobia or with its history in past ages, we cannot find it. It does not appear anywhere that he has ever seen a case of this sort, or even studied the descriptions which others have given of it." No method can be considered of any value unless it stands the following experimental test:—

1st.—The dogs from which the virus is obtained must be proved to be rabid.

2nd.—Carefully ascertained quantities of the saliva must be injected into animals, and in a certain proportion of the cases the proposed method must be adopted.

3rd.—Control experiments must be repeatedly made.

4th.—The results must show a very great and constant difference in favour of the method.

Now, has Pasteur conducted such a series of experiments? There is no evidence whatever to show that he has ever attempted it. I propose subsequently to discuss the fallacies which seem to underlie Pasteur's conclusions derived from the "cases" in the human subject.

II.

M. PASTEUR'S CRUCIAL CASE.

I WILL here examine the "cases" on which M. Pasteur depends for the value of his method. The first case was that of Joseph Meister, aged 9 years, bitten by a dog on July the 4th, 1885. The bites were numerous and severe. The principal ones were cauterized the same day with carbolic acid. On the 8th of July, at 8 P.M., M. Pasteur, in the presence of MM. Vulpian and Grancher, inoculated half a

syringeful (Pravaz) of bouillon, containing a fragment of a spinal cord of a rabbit dead from hydrophobia on June the 21st ; and, from the 6th of July, thirteen injections in all were made, each day a fresher cord being used. It was subsequently discovered that the cords used between the 6th and 10th of July were non-virulent, and although it is said that the cords used on the 11th, 12th, 14th, and 16th were all virulent and increasingly so, nothing is said about that which was used on the 13th. I will not now stop to enquire whether the circumstance that more than half of the cords used for protective purposes were subsequently discovered to be perfectly inert is one which is calculated to afford additional confidence either in the value of the material used, or in the precision which has been claimed for the operation ; but will proceed to discuss the evidence on which Pasteur relies in confirmation of his theory, for I regret to say that I can call it nothing more substantial. The points relied upon are two : (a) That at the *post mortem* examination of the body of the dog which inflicted the bite, the stomach was found to contain "hay, straw, and bits of wood," and therefore it is concluded that the animal was rabid. (b) That Drs. Vulpian and Grancher, who were called by M. Pasteur to see the boy, said, he was "almost inevitably exposed to contract hydrophobia *in consequence of the severity and the number of his bites.*"

First as regards the dog. Is the presence in the stomach of "hay, straw, and bits of wood" any evidence whatever of hydrophobia ? While it is true that dogs suffering from hydrophobia sometimes have perverted appetites, so have dogs which are merely half starved. The contents of the stomach of a dog are no more diagnostic of hydrophobia than is the chlorotic girl's depraved appetite for slate-pencil and cinders indicative of insanity. In

the absence, therefore, of any other facts indicating rabies, this fact, on which such importance has been placed, simply counts for nothing. But supposing even that the dog was suffering from hydrophobia, the boy's escape is explainable on other grounds than those set forth by M. Pasteur. We have already seen that the risk of infection from the bites of a rabid animal is, under ordinary circumstances, only one in four.

Secondly, as regards the boy, on what grounds is the supposition based that he would have been the victim of hydrophobia but for the treatment? The two medical men who saw the patient say that he was "*almost*" (the italics are mine) "inevitably exposed to the disease." Why? Because of "*the severity and the number of his bites.*" Ignoring altogether the important qualification "*almost*," which really destroys the value of the case scientifically, the severity and the number of the bites constitute a condition which possesses no diagnostic signification whatever, and it is somewhat remarkable that M. Pasteur should produce this evidence in favour of his treatment, when, as we shall see hereafter, he subsequently relies on that very condition as explaining the unfavourable termination of a case of wolf-bite.

On the other hand, it is not unreasonable to suppose that a severe bite attended by considerable bleeding may be less dangerous in its effects than a slighter one attended by little bleeding, in consequence of the resulting dilution and possible expulsion of the virus by the hæmorrhage—the wound, as it were, washing itself. But be that as it may, we have the further statement that the boy's wounds were cauterized the same day with carbolic acid, a proceeding which, if properly and thoroughly conducted, is the very best known preventive treatment against hydrophobia, and one which not only common sense suggests, but which has received the

sanction and encouragement of the best authorities on the subject. Mr. Youatt states that he has employed lunar caustic to upwards of four hundred persons, and four times on himself, after bites from dogs decidedly rabid, and he has not seen the disease appear in a single instance ("Holmes's Surgery"). Dr. Cunningham, of Chicago, who is said to cauterize about one hundred and twenty dog-bitten persons annually, stated that the mortality averages about three in that number. Then, again, there is a further objection to M. Pasteur's assumption that the boy "escaped the hydrophobia which his bites *might* (the italics are mine) have developed." In spite of his confidence in his treatment in this test-case, he substitutes the mild word "might" for "would." But supposing the boy to have been in any real danger, it is far too soon for M. Pasteur to come to any such conclusion. The boy was bitten on the 6th of July 1885, not yet a year ago; but the period of incubation or latency has been known to extend to twelve months and frequently to over two years; though it is true that the average time is considerably less. In the published cases we look in vain for the slightest evidence of rabies in the animals which inflicted the injury, except in the fatal cases. Throughout we meet with the same claim to success and the same fallacies in support of it; the same cold shoulder is offered to the *cons*, while the *pros* are complacently cuddled. In fact, when you have considered and criticised the test-case, you have disposed of them all. This is a fitting place to mention the excuses which have been made by M. Pasteur for the failure of his treatment in those cases which have terminated fatally. In the first fatal case it was said that, owing to the child having been bitten on the head, the chances in favour of his method were greatly reduced. A lame excuse, seeing that it has been distinctly proved by Dr. Dujardin Beaumetz of the

Academy of Medicine of Paris, of which M. Pasteur is a member, that there is no relation between the bitten part and the period of incubation. In the second fatal case it was said that the period between the infliction of the bite and the occurrence of the disease was too short,—*viz.*, sixteen days,—to admit of efficient treatment. But he had asserted that if a year had elapsed since the bite, he could save the victim.

In the third fatal case it is said that the bites were too deep ; an equally lame excuse, since the depth and severity of the wounds bear no relation to the period of incubation ; and his treatment is supposed to be rapid and to pervade the whole system. Here, then, after infallibility was claimed for the treatment by M. Pasteur in these words, “he could ‘dare’ to call his method ‘perfect.’” And, again, “I am confident my treatment will be successful if commenced at any time before actual hydrophobia sets in, even if a year or more elapses between the bite and the commencement of treatment.” We have now a confession of utter impotence under these conditions : *Firstly*, when the wounds are about the head ; *secondly*, when a longer period than sixteen days has elapsed since the bite ; and, *thirdly*, when the wounds are deep and severe. How soon shall we be enlightened as to other conditions under which this marvellous method of treatment is worthless ! It is simply astonishing that, on the mere *ipse dixit* of M. Pasteur, in the face of the stern logic of facts, and the inherent improbabilities of the utility of such an extraordinary and elaborate system of treatment, requiring, as it does, an enormous and constant sacrifice of animal life, scientific bodies should have so blindly accepted as Gospel truth all that he has chosen to promulgate. Nothing but the great name which he has earned—and hitherto deservedly so—can account for this

wonderful and unscientific credulity. I do not for a moment assert that the statements of such a man are not entitled to the greatest weight ; but I do insist that no decision should be arrived at without a calm and unbiased consideration of the facts, to the exclusion of hysterical sentimentality and the mere assumptions of any authority, however eminent.

It is simply monstrous that "Institutions" should be started in America and on the Continent for the sacrifice of thousands of animals for the *supposed* cure of victims, who, after all, do not amount to more than three in a million of the population of those countries. Two facts alone stand out and plead alike for the man and the beast that the hand may be stayed. One is, that M. Pasteur's method is admittedly not free from danger to the person seeking relief ; and the other is, that, even assuming that, in some of his cases, the dogs inflicting the bite were rabid, in no single instance can it be said that sufficient time has elapsed since the injuries were inflicted to enable M. Pasteur, or anyone else, to assert that the patient is out of danger.

III.

M. PASTEUR'S "CASES."

WE now learn that, in April last, M. Pasteur read the following report at the meeting of the Academy of Sciences, Paris :—" The number of persons so treated amounted, up to the 12th instant, to seven hundred and twenty-six, including those who are still undergoing treatment ; of this number there were six hundred and eighty-eight who were bitten by mad dogs, and thirty-eight by wolves, the latter being all Russians. The patients belonging to the first category are, with the exception of the little girl Pelletier, who, it will be remembered, died after a few inoculations, all doing well. More than

half that number have passed the dangerous period. Of the thirty-eight Russians who have been treated and are still undergoing treatment, three have died rabid;* the others are doing well, but it is impossible to foresee what may happen to them, as there exists a profound difference between the bites of dogs and those of wolves, the proportion of deaths caused by rabid wolves being at least 82 per cent." Then again he states that the duration of incubation of human rabies, caused by the bite of a rabid wolf, is often very short, very much shorter than rabies after the bite of a mad dog; and that the mortality after the bites of rabid wolves is considerable, if we compare it with the effects of the bites from dogs. Now let us examine these assertions, which, "if true," would be most strongly corroborative of the validity of M. Pasteur's claim to having discovered a prophylactic against hydrophobia. The first question which presents itself in the enquiry is, What warrant has M. Pasteur for the emphatic assertion that the seven hundred and twenty-six persons who are said to have been injured were bitten by mad animals? It is impossible that he can have been personally aware of the fact that all the animals were rabid, and we must assume, therefore, that he has been told so, and further, that he has believed what he had been told without due enquiry. We have already seen that in the crucial case there were the following flaws:—

(a) There was positively no evidence that the dog which inflicted the bites was rabid, and the "assumption" that it was so was based on insufficient grounds.†

(b) The boy's injuries were no indication of the rabid condition of the animal, consequently the

* One more has died since.

† *Vide* "Concluding remarks" for a further reference to the case of Meister.

assumption that he might have suffered from hydrophobia is worthless.

(c) That more than half the material which was used for the inoculations, and upon the activity of which, according to M. Pasteur, depended the success of his method, was discovered subsequently to be inert. What guarantee is there that, in other cases, all the matter used was not inert? These inoculations have now become so numerous and frequent, that it is simply impossible that the cords could have been submitted to the test as in the crucial case.

(d) The boy's wounds having been cauterized, immunity from hydrophobia up to this time may be due to that fact.

(e) Hydrophobia does not necessarily follow the bite of a rabid animal.

(f) The period of incubation may extend to years, so that it is yet too soon to assume that the boy—if inoculated with rabic virus—has escaped.

We have seen, therefore, that M. Pasteur is not justified, in the case of the boy Meister, in making the positive statement that he was bitten by a mad animal; but let us see whether he has any more justification in some other cases which have been made public. The New York correspondent of the *Lancet* writes (Feb. 1886):—"The annual hydrophobia scare has broken out in this vicinity with its usual severity. What gives this epidemic peculiar interest is the craze of the community to send all who have been bitten to Paris to be inoculated by Pasteur. . . . Several children in Newark were bitten at the same time by a dog 'supposed' to be suffering from hydrophobia. Four of the children were immediately sent to Paris, where they were duly operated on by Pasteur. The results of the vaccinations were pronounced by him to be entirely successful, and the children have returned to their houses. They have been examined by their physician, who

declares they are in good health. The other children, two of whom were severely lacerated by the same dog, and who remained at home, are also well." Dr. Dulles mentions two other Americans who went to Paris to be inoculated. A man named Kaufman, and a boy named Edward Bucklin, were bitten by dogs, "but there was not the slightest evidence to show that the dogs were rabid." The Paris correspondent of the *Lancet*, who at first appears to have been a believer in M. Pasteur, writes in March last: "It struck me, however, that many of the applicants had been driven there more from fright than necessity, as in many cases they did not wait to ascertain whether the dogs that bit them were really rabid. *In some cases they were not even bitten.* For instance, *I saw a young English lady who submitted to the operation simply because she had been licked in the mouth by her dog, which had no signs of hydrophobia.*" He states, further: "I met there also Dr. Hime, of Bradford, who came over with his party of seven persons, consisting of four little boys, a little girl, a youth, and a man, who, Dr. Hime told me, were bitten, on January the 24th, by a dog which was ascertained to have been indubitably rabid, as another person who was bitten at the same time by the same dog died soon after from hydrophobia." This case would at first appear to be a rather strong one in favor of M. Pasteur's method, but let us test it by what is stated in regard to it by the Paris correspondent of one of our Calcutta papers, who has watched M. Pasteur's operations with keen interest, and has shown considerable acumen and intelligence in dealing with the facts. "There was one of the Bradford children bitten on the eyelid, where deep cauterization was impossible, but since I have got at the facts connected with Ashworth's death from rabies, I do not understand how the dog which bit him can possibly be identified with the one which bit the

children, the tailor Gibson, and the Irishman Jarvey. So far as I can ascertain, Ashworth was bitten after nightfall, and from behind. He had his hands behind his back as he was walking home, and a dog jumped at them and bit one. But he had talked so little about the matter that his sister had 'to think' before she remembered the circumstance of his having *five weeks previously* (italics mine) paid a chemist's bill for getting a finger cauterized." Again, the same correspondent writes : "I have seen many absurdly frivolous cases up for treatment at the Rue d'Ulm. The whole family of Austin Key, the London doctor, including sons, a daughter, grand-daughter and daughter-in-law, came here to be inoculated, because a dog, which they think must have had rabies, *licked their hands and faces.*" And yet, in the face of these facts, which ought to have been known to M. Pasteur, he asserts positively that the seven hundred and twenty-six persons inoculated were bitten by *rabid* animals. As to the statement that the period of incubation after the bite of a rabid wolf is much shorter than after the bite of a rabid dog, it is not borne out by facts. Rabies has been known to occur the day after the bite of a rabid dog (Troillet), whereas there is no known instance of the kind in the case of the bite of a rabid wolf. There is one sentence in M. Pasteur's report which is unquestionably true, though I doubt whether he meant it to be read literally. He says :—"The mortality after the bites of rabid wolves is considerable, if we compare it with the effects from the bites of dogs." Precisely, and hence one of the reasons of his want of success in the case of rabid wolf-bite, and his complete success in ordinary dog-bite. As I have already pointed out, there is another and important factor which has materially assisted in giving an appearance of success to M. Pasteur's method, — namely, cauterization.

In this connexion the correspondent I have quoted observes: "I wish to believe in Pasteur's curative powers, and have sometimes thought I believed; but there are many things which prevent absolute faith. I cannot help asking myself whether his alleged cures are well established. Most of the hundred and fifty patients whom I have seen being operated upon in his laboratory were cauterized almost immediately after they were bitten by dogs or wolves. Might they not, therefore, have recovered without Pasteur's intervention?" These are the words of a lay writer, and it is marvellous that there should exist such blind infatuation among professional bodies as to ignore such reasoning. There may be some who regard the one fact that M. Pasteur has had only four deaths out of seven hundred and twenty-six persons treated by his method as strong *primâ facie* evidence in his favour, but that is to be met by as remarkable statistics—not only in reference to this subject—for example, Youatt's four hundred cases without a death, but also in regard to snake-bite. The returns of the sickness and mortality in the United States Army during the War of the Rebellion prepared from information furnished by Army Surgeons, show that there were two hundred and thirteen cases of snake-bite with only two deaths. ("Medical and Surgical History of the War of the Rebellion," First Part—Medical, pp. 636, 710.) A startling piece of evidence in favour of any particular nostrum, *had it only been administered!* I am very much afraid that M. Pasteur would not subscribe to this:—*Ista veritas etiamsi jucunda non est, mihi tamen grata est.*

IV.

M. PASTEUR'S PROPHYLACTIC MATERIAL.

THERE is one very important point which I have failed to bring into sufficient prominence, and my casual allusion to which may be open to misinter-

pretation. I have stated that inoculation with M. Pasteur's prophylactic was admittedly not free from danger to the person seeking relief. Let us see what M. Pasteur has said in this connexion. With reference to the case of the boy Meister he says : " The death of this child appearing inevitable," he decided, " not without keen and cruel solicitude," to try his experiments, which, as we have seen, was from his point of view, a complete success. After treatment the boy was sent home " cured," having, remarks M. Pasteur, escaped not only the hydrophobia which his bites might have developed, but that " with which I had inoculated him to test the immunity due to the treatment, a hydrophobia more virulent than that of street dogs."

Apart from the laxity of the phraseology, let us enquire into the meaning of this quotation :—" The boy having been bitten by a rabid dog, was in danger of an attack of hydrophobia, but was saved from that attack by the inoculation with a cultivated though more virulent rabic virus." But what, may I ask, would have been the result of inoculation with this terrible virus if the boy had not been bitten by a rabid dog, and was, therefore, in no danger of being attacked with hydrophobia? We have already seen that, prior to M. Pasteur's intervention, this very case was open to the gravest suspicion of being free from the elements of danger—a suspicion which must have been shared by M. Pasteur himself, or why this " keen and cruel solicitude " about inoculating with his virus? What did he fear? Why that, if, by the bite, rabic virus had not been deposited in the tissues, his inoculations would give rise to hydrophobia. Accepting his statements as to the power of his prophylactic material, we are, of course, driven to the conclusion that the boy was unquestionably bitten by a rabid animal, and was in real danger from the bite, or

symptoms of hydrophobia would have manifested themselves as a consequence of the "protecting inoculation."

Does M. Pasteur really believe that inoculation with his material is attended with the slightest possible danger? If he does, he showed a disregard for human life in his subsequent operations, which is hard to reconcile with his profession of "keen and cruel solicitude" in the case of the boy Meister. Does not the readiness with which the operation was had recourse to in these seven hundred and twenty-five cases indicate rather a freedom from belief in the virulence of the protecting material? M. Pasteur, therefore, is convicted either of using a material which does not possess the power he has attributed to it, or of using a material which is more virulent than that of the "hydrophobia of street dogs" in a reckless manner, to the imminent danger of the lives of the poor creatures who resort to his laboratory in the hopes of being saved from danger. I think I may say, without presumption, that what I have written will assist my readers in arriving at a verdict, but, whatever conclusion may be arrived at, it must be conceded that M. Pasteur's discovery is scarcely likely to add to the brilliancy of his reputation.

I have now advanced quite sufficient, and in language not any too strong, to show that M. Pasteur's method of treatment, so far as the world has been enlightened, rests on no firmer basis than that which justifies the vaunted powers of "Holloway's Pills" and "Mother Seigel's Soothing Syrup."

V.

CONCLUDING REMARKS.

SINCE the above was written, a very able letter on the subject, which appeared in the *Standard*, from the pen of Dr. Anna Kingsford, has come under my

observation ; and it is very strongly confirmatory of the correctness of my criticisms of M. Pasteur's method. It appears that Dr. Anna Kingsford had been giving instances of persons who had been needlessly treated by M. Pasteur, and amongst others she mentioned that of Mr. Hughes, a medical man, whereupon Mr. Hughes writes :—"Although the veterinary surgeon in attendance gave it as his opinion that the dog which bit me was not suffering from rabies, I entertained grave doubts both before and after the animal's death, as to the correctness of his judgment. . . . I was then at some trouble to verify or dissipate my doubts, and finding these more than corroborated, I felt myself justified in placing myself in the hands of M. Pasteur." I wonder whether Mr. Hughes was told by M. Pasteur, before submitting himself to be operated on, that if he were not bitten by a rabid animal, he ran the risk of contracting hydrophobia by the inoculation with a material more virulent than "the hydrophobia of street dogs"! Another question : were Mr. Hughes's wounds cauterized ? Mr. Hughes says he was confirmed in his opinion that the dog which bit him was rabid, by that of some eminent veterinary surgeons whom he subsequently consulted ; but as Dr. Anna Kingsford very pertinently remarks :—

"It is, of course, natural enough that Mr. Hughes should endeavour to vindicate the step he thought fit to take in regard to his accident. That he sincerely believed, and may still believe, his alarm justified, I do not dispute. But it must be obvious that a diagnosis based on competent experience and on observation of the dog while alive, completed by a professional *post mortem* examination such as that recorded in detail by Mr. Richard Hughes, M.R.C.V.S., must carry more weight to reflecting minds than the mere opinion on hearsay of any number of

veterinary surgeons, however eminent. Most of the symptoms of rabies are liable to simulation by other acute canine disorders ; and herein lies the great difficulty of certainty in regard to it. Foaming of the mouth, excessive irritability, wildness of manner, morbid appetite, and other rabid symptoms are all present in maladies from which dogs, especially when young, are apt to suffer—maladies which, however, bear no relation to hydrophobia, and are incapable of developing its specific contagium.”

But supposing the dog was mad, the fact might justify Mr. Hughes in resorting to Pasteur if he was ignorant of the real merits of his method, but it by no means follows that the method has been in any way instrumental in protecting Mr. Hughes from an attack of hydrophobia. If his wounds were cauterized—as there is every reason to believe was the case—immunity—if, indeed, it is not very premature to talk of that—may be due to that proceeding, or he may not have been inoculated with rabic virus. Mr. Balfour, Inspector-General of Hospitals, Her Majesty's Indian Service (retired), mentions one of the most convincing cases in favour of cauterization in the bite by rabid animals that was ever placed on record. He relates how eighteen or nineteen persons were bitten by a rabid dog. One of these came under Mr. Balfour's observation ; he had been badly bitten in the leg. After placing the man under the influence of chloroform, Mr. Balfour excised and cauterized the wounds with a red-hot iron. *This man was the only one who escaped.** All the others who had not been cauterized died from hydrophobia. This case is unique, and the number of cases of hydrophobia from the bite of one animal is unprecedented. On the other hand, I now relate an instance that is

* Vide *Lancet*, Vol. II, 1877, p. 599.

within my own knowledge of a case shewing the uncertainty of inoculation after the bite of a rabid animal. A *byragi* (religious mendicant) and two coolies were attacked by a stray pariah dog, and all were bitten. None of the wounds was cauterized. The mendicant was attacked with hydrophobia four months after being bitten, and he died within three days. Neither of the others has been affected, and it is now nearly thirteen years ago. But to return to Dr. Anna Kingsford's letter. It will be observed that that lady's remarks dovetail in most accurately with my observations setting forth the weakness of Pasteur's so-called method. Dr. Anna Kingsford continues :—
“There can be no possible doubt that a vast number of the patients now thronging M. Pasteur's laboratory are precisely in the position of Mr. John Hughes. They have been bitten by animals either sickly or ferocious, and seek inoculation as a safeguard, in case they may have incurred danger.

“The record of one such case now lies before me. It is that of Mr. Taylor, who was bitten at Worcester by a St. Bernard dog, and who was conducted to M. Pasteur about a week ago by Mr. Muddell. Mr. Taylor admits that the dog in question was not rabid, but that, nevertheless, he resolved to be inoculated by M. Pasteur, so as to avert possible peril and calm his own alarm. It is now placed beyond reasonable doubt that the famous Newark mad dog which bit the four children sent from New Jersey for inoculation was not rabid. In the *New York Times* of the 2nd instant, particulars concerning the pricking of this bubble are given in emphatic language, and at a length which precludes them from reproduction here. Yet this case was unhesitatingly included with that of Mr. John Hughes, by M. Pasteur in the report made by him to the Paris Academies of Sciences and Medicine. It is not my object to impugn the veracity of any individual. I

seek only to call public attention to the fact that, so far as we yet know, not one of the cases which have been prominently brought before us as successful examples of M. Pasteur's treatment has been scientifically verified as a test-case, the evidence being dependent either on the patient's own opinion in regard to the character of the bite, or on surmises or diagnosis based on descriptive reports afforded by the patient and by witnesses more or less ignorant and alarmed.

"In the case of Meister, as in that of the Newark children, it appears that the evidence for the rabidity of the dog has broken down. I am informed that, on the same day, the dog in question bit three persons, one of whom, a boy named Max Vonne, was neither cauterized nor sent to M. Pasteur. Yet this lad continues in good health, as also does the proprietor of the animal, who accompanied Meister to Paris, but whose wounds M. Pasteur declined to treat, not deeming them sufficiently important."

The last clause certainly conveys a remarkable piece of information when we bear in mind the readiness with which he inoculated others who had nothing nearly so substantial to exhibit as evidence of the infliction of a bite of a rabid animal. How could M. Pasteur possibly judge of the importance of a wound by its extent. Rabies frequently follows a most insignificant wound, a mere abrasion, a scratch. He was not quite so particular about some of the other cases, although there was such a risk of inoculating the victims with "hydrophobia more virulent than that of street dogs."

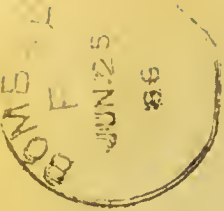
* THE *Lancet*, 15th May 1886, just to hand, contains an account of "A Day with Pasteur," by Dr. G. H. Brandt, and as it is written evidently by a friendly hand, we are justified in concluding that M. Pasteur has been correctly reported. After

* This was written after the above had gone to press.

describing what he saw, Dr. Brandt says: "As we were questioning him, a paper was shown him in which there was a paragraph which said, 'we place no reliance on M. Pasteur's work, as many cases of rabies appear many months—eighteen, twenty and even twenty-four—after the bite of a mad dog.' To this M. Pasteur answers: 'This is not good reasoning, for statistics shew that the great majority of cases have an incubation period of from twenty to sixty days. Cases of rabies happening before the twentieth day are very rare, and those after the sixtieth day are still more rare, so that our treatment includes the majority of cases.'" Cases of what, may I ask? Of undoubted and proved bites of rabid animals? Or of assumed ones? The reasoning in the former instance may be weak if it stands alone, but it is certainly stronger than M. Pasteur's. After quoting the case of Meister, M. Pasteur — according to his reporter — remarks: "Statistics also show that there are about fifteen cases annually of hydrophobia admitted into the Paris Hospitals followed by fifteen deaths. Since we have begun our treatment there has been only one case admitted; this case was not inoculated, and death was the result." To this I reply that the statistics which I have had the opportunity of seeing do not bear out this assertion. During the five years, 1872-77, in the whole Department of the Seine, there were only 36 deaths, or a little over an average of 7 per annum (*Lancet*, Vol. II, 1881, p. 1012). And in 1880, in the same Department, there had been only two cases up to the end of September (*Lancet*, Vol. II, 1880, p. 507). But even if we accept his statistics as accurate, he takes the average number for the years as the actuals, a line of reasoning which is not only bad but positively misleading. It is a well-established fact that the mortality from rabies, like the morta-

lity from many other causes, fluctuates considerably year by year. "To the unbelievers," says Dr. Brandt, "M. Pasteur says : 'Wait ; time will reveal many facts connected with this question ; and it is only by continued experiments and constant observations, carried on for a considerable time on hundreds of cases, that we shall be able to arrive at positive and definite results.'" Well, may we rub our eyes and exclaim "what, are we awake, or do we dream ? Are these the words of the man who, long before he possessed his present experience, could '*dare*' call his method perfect." Are these the words which would have brought down upon him the showers of honours with which he has been drenched ? Does he tell the scared creatures who flock to him that they are being experimented on to enable the world of unbelievers to arrive at positive and definite results ? The unbelievers asked him for proof, and he has advanced mere assertions and assumptions. They have asked him for bread, and he has thrown them a stage loaf. Now, he says : "Wait ; positive and definite results can be obtained only after continued experiments are carried on for a considerable period." But the unbelievers know by this time quite enough of the elasticity of M. Pasteur's logic to convince them that he sniffs the enemy in the field. It is a curious coincidence that a very able Commission has been deputed to France to examine and report on M. Pasteur's method of treatment ; and as it is unlikely that the members of the Commission will have the opportunity of "continued experiments and constant observations, carried on for a considerable time on hundreds of cases," an adverse decision can be met by Pasteur's last shot :—"Compare their experience with mine." M. Pasteur reminds one of the "boneless one," he can adapt himself so easily to any position. No

doubt, however, the enquiries of the Commission will include exact experiments on the lower animals, whereby sound results are likely to be obtained, and that they will be duly published, so that the profession will be enabled to judge for themselves. Anyhow, the names of the gentlemen forming the Commission are a sufficient guarantee that something more substantial than mere assertions and assumptions, unreliable statistics, and fallacious reasonings will be forthcoming ; and that, whatever may be their decision, it will be entitled to, and will receive, the respect and confidence of our profession.



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